KING (J.)

ANNUAL ADDRESS

DELIVERED BEFORE THE

MEDICAL SOCIETY OF PENNSYLVANIA,

AT THE MEETING HELD IN PITTSBURG, JUNE 12, 1867.

BY

DR. JAMES KING,

PRESIDENT OF THE SOCIETY.

Extracted from the Transactions of the Medical Society of the State of Pennsylvania, June, 1867.

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Gentlemen of the Medical Society of Pennsylvania:

The constitutional provision of this Society which requires the presiding officer to open the annual session with an address, furnishes a good opportunity for offering such suggestions as he may consider useful in advancing the general interests of medicine. No particular subjects are prescribed. But by the custom established we have, heretofore, had discussions on medical education, the advantages of medical organization, medical ethics and their kindred themes, all designed to exalt the claims of our profession and to promote a higher standard of qualification, a more honorable bearing, and a greater devotion among its members.

The progressive qualification of Medical men is indissolubly associated with strict integrity, generous action, and self-sacrificing labor as a means of elevating their character as a professional body, and may well be made the object aimed at on an occasion like the present, whatever the topic selected for remark. So many practitioners contenting themselves with the knowledge to be derived from the text-books recommended to them as students, it will be well, as I conceive, if an argument can be made to convince them of the necessity for their more thorough investigation of the great doctrines which lie at the foundation of our science. I admit that we may learn from text-books the commonly received views of most important medical questions; but we must go beyond these if we would

take our rank as men of science, having views of our own. If we would apprehend medical truth in all its bearings and make it of the highest practical value, we must trace it through its original development, weighing all the facts for ourselves—not so much following the deductions of text-books as making our own deductions. Who can have such knowledge as a physician, whether specialist or general practitioner, ought to have of the composition, uses, and morbid changes of the blood, of the proliferation, constitution, and growth of cells, or of the functions and diseases of the nervous system, simply by the study of text-books on physiology and practice. Such great questions as these require, in order to a right understanding of them, much patient research, and are so important that, without due attention to them, there can be no eminent qualification in a physician. Therefore, at the risk of being thought unfortunate in the selection of a subject, I propose to speak of the importance of a knowledge of the nervous system.

The first thought that occurs in this connection is in regard to the amazing advances made in this branch of Medical Science within the present century. Going back to Bichat at the beginning of this period, we find the first clear exposition of the distinctions between the nervous systems of organic and animal life. But he was not able to discern the line of physiological separation between conductors and centres of either system as afterwards demonstrated. Although he wrote much as the older physiologists had done, of sympathy, as of "two nerves of the same pair sympathizing with each other," or "two nerves of the same side sympathizing without belonging to the same trunk," he had no idea of the reflex function of the nervous system upon which sympathy depends. Yet he approached very near the spot from which this great truth might have been observed. We find him cutting the œsophagus of a dog, to ascertain whether or not the irritation of the uvlan

which causes emesis is transmitted by the mucous membrane and contiguous tissues or by either; and perceiving that "notwithstanding the solution of continuity" the animal made efforts to vomit as before, he acknowledged his ignorance of the cause of the sympathetic act. If he had made one cut more, severing the glosso-pharyngeal nerve, the mystery of "sympathy" would probably have been revealed to him, and his researches, *splendid* as they were, would have blazed with a new illumination. Yet it may be that this truth could rise to view only in its logical order.

We know that other great physiological conclusions were reached before the question of sympathy was susceptible of a rational solution. First, Fletcher made the important announcement of the distinct offices of the vesicular and tubular neurine—that the former is connected with the production of the *vis nervosa*, receiving and reacting on impressions, while the latter is concerned in the nervous manifestations only as conductors. Next, these conductors were found to be divisible into two sets. Sir Charles Bell having shown that in the same cord there are conductors for sensation and orders of the will to muscles, totally distinct from each other, and transmitting impressions and impulses in opposite directions. A beautiful revelation this—which he so ably illustrated in his demonstrations on the 5th pair of nerves, and which has been settled as a positive truth by the experiments of that renowned vivi-sector Magendie, on the anterior and posterior roots of the spinal nerves.

Then followed that great accession to our knowledge of the nervous system made by Marshall Hall. His convincing illustrations of, what he was pleased to call, the excito-motory system, called by others the reflex function of the spinal cord, consisting of a series of centres in the spinal medulla with their excitor and motor nerves, which preside over acts totally distinct from sensation and voluntary motion, have been abund-

antly confirmed by the researches of Reid, Grange, and other co-laborers. And they have not only revealed the mystery of sympathy, but have "so far changed the whole aspect of this department of Physiological science, as to render it necessary," as Carpenter says, "for those who had previously studied it, to begin *de novo*."

With the keys to the secret chambers of the nervous system furnished by the labors of Bichat, Fletcher, Bell, Hall, and others referred to, we find treasures of knowledge continually being brought to light. Experiments on living animals have been so added to observations in pathological cases as to develop the subject still further; and thus our neurological literature has been vastly enriched with discoveries of the highest practical and scientific interest.

It is only necessary to glance at the investigations and conclusions of physiologists since the announcements of Marshall Hall to appreciate the tendency to progress in this department which characterizes our time. We find Longet and Matteucci inquiring into the relation of electrical phenomena to the nervous system, and proving that no electrical current is inherent in a living nerve—that electricity is not the active force residing in nerve tissue, but appears in it only as a secondary effect. We find others resorting to galvanism as a delicate test for detecting and exposing the special function of particular nerves. Thus Pflüger, by experiment with galvanism on the spinal and splanchnic nerves in the rabbit, shows how by an exciting agent the intestinal "motus peristalticus" is arrested, and so demonstrates an inhibitory function, if not a distinct system of inhibitory nerves. So the different or antagonistic function, if I may so designate it, of the vagus and of the nerve proceeding from the inferior cervical ganglion of the sympathetic, are shown by their galvanization—the former arresting, while the latter accelerates the pulsations of the heart as first observed by Weber. In a similar manner the

long vexed question in regard to that property styled by Haller the "vis insita" has been determined by showing it to be a property of muscular fibre and not of the nerves. It is true the experiments of Matteucci and Wundt could only be considered plausible evidence of this; but it was conclusively established when Bernard, Kölliker, and Althaus found that galvanism would excite muscular contraction after the motor nerves had been completely paralyzed by the poison of woorara.

Among the recent contributious to science we may consider as eminently worthy of notice, the researches of Claude Bernard in regard to the influence of the nervous system on calorification, secretion, and modification of the blood. His illustration, however, of its influence on calorification, as shown by the effect of section of the 5th pair of nerves in reducing animal heat and of section of the sympathetic in augmenting it, in the parts which these nerves supply, seems not to have been admitted or appreciated by his great contemporary Brown-Séquard, who attributes the increase of temperature in the latter instance solely to the greater afflux of blood to the parts which the section has paralyzed. But Bernard has clearly shown by experiments on the rabbit's ear, that stasis of blood produced by obstructing the veins will make the parts colder, and yet, on division of the cervical sympathetic, the temperature will immediately be raised. And any one who will carefully weigh the arguments presented by these distinguished experimenters will hardly fail to accept Bernard's conclusions on this point as correct.

The bearing of the proofs which Bernard has developed respecting the influence of the nervous system on secretion is not so much questioned. There is one problem which they are admitted by Brown-Séquard as solving perfectly—the problem involved in the facts that secretion may be increased by section and paralysis of a nerve in one set of cases, and by the opposite means of excitation in certain others—facts which

had seemed to present an "inexplicable contradiction." Another problem which involves the most practical considerations, if not solved perfectly, is put by Bernard's experiments very far on the road to a solution—the agency of the nervous system in regulating the presence of sugar in the blood. The arrest of the production of this sugar which he has shown by section of the nerves of the liver, and the augmentation of it by excitation of the same nerves, may be regarded as the most important step yet made towards the elucidation of that hitherto mysterious and intractable malady "diabetes mellitus." Bouchardat and Magendie had found sugar in the blood; but it was reserved for Bernard to show that by a nervous agency it might be increased or diminished.

The remarkable feature of Bernard's revelation is—that we have chemical phenomena, such as changes of temperature and such as the production of sugar out of material to which, so far as our analysis can show in some cases, no saccharine element has been furnished—such chemical phenomena produced by nervous agency, constituting modifications in the composition of the blood, into which the nerves do not penetrate. To show how direct this influence on the blood is, it is only necessary to consider the effect on its coagulability produced by division of the cervical sympathetic in the horse. Bernard calls attention to the fact that the blood of the horse coagulates much more slowly than that of other warm blooded animals, and the clot formed, presents more of a buffy coat. But the blood drawn in half an hour after the division of the sympathetic, he informs us shows no buffy coat, while the coagulation that, before the division, takes place very tardily, occurs afterwards almost at once. In facts like this we may possibly find the explanation of a circumstance connected with some cases of sudden death. Having found the coagulability of the blood to increase from section and paralysis of a nerve, it is possible we may find fluidity to occur from the opposite

agency of excitation, and the fluidity which characterizes the blood in death by lightning may be more the result of the direct influence of the electrical current on the nervous system than on the circulating fluid.

But it is not my purpose to speculate on the new views of such eminent physiologists, nor even to refer to all that have been advanced either by Bernard or others. We would find more that is new and interesting, than could be noticed in a brief address, in the works of Brown-Séquard alone. His brilliant demonstrations of the functions of the spinal cord—showing with such clearness the course and place of decussation of conductors for transmitting sensitive impressions; his proofs of distinct conductors for different kinds of sensation; his discovery in the central gray matter of these conductors, hitherto believed to be in the white posterior columns; his happy elucidation of the obscure phenomenon named by Magendie, "recurring sensibility;" his scientific method of diagnosis of the various lesions in paraplegia, with the rules for therapeutical applications founded on the physiological effects of remedies; his exposition of the pathology of epilepsy and his practical suggestions in regard to various other functional nervous diseases—all these, and much more, belong to his extensive contributions to our science, contributions which exhibit the transcendent genius of a man worthy to be ranked among the first medical philosophers of any age.

However, I would not place all the conclusions arrived at, plausible as they may be, in the category of positive demonstrations. Some will no doubt prove to be too hastily made. Of some the absolute fallacy may appear from future investigation. Such has been the fate of doctrines which were well received. Flourens' doctrine, for example, of the function of the cerebellum as the co-ordinator of voluntary movements has been incorporated in text-books as sufficiently established; but great doubt has been thrown upon its correctness by recent

experiments. The power of co-ordination in pigeons, as found by Professor Dalton and others, is only temporarily lost when the cerebellum is destroyed. The same may be said of the "vital knot," as proposed by Longet—that is, the point in the cerebro-spinal axis which he held could not be touched without instant death; or his doctrine regarding the tuber annulare as the centre of sensation and voluntary motion. These, though resting on experiments thought to be conclusive, have not borne the test of Brown-Séquard's examination, who claims to have exposed them as errors.

Having sufficiently referred, as I think, to the great progress made in neurology in our time, and so far shown it to be an important subject, we may next examine its importance in the light of practical benefits to be derived from the knowledge to which we have attained.

The great mystery which has overshadowed many points in the physiology of the nervous system affords the proper explanation of the obscurity which has enveloped the approach of the most formidable of the nervous diseases—such as the insidious diseases of the brain. These have been called insidious, from having advanced far towards a fatal disorganization before the medical eye detected their existence—and they made this progress unperceived, because our former ideas led us to look to perversions of sensation, general or special, or to loss of muscular power, or to some fixed derangement of intellection as the only reliable evidences of lesion in the brain. But we have learned from physiological research, that the most extensive lesions may exist in this organ and no such evidence appear; so that we must look to other sources for information. Immense masses of brain, as shown by experiment, may be disturbed, lacerated, galvanized, cauterized, and cut away without involving sensation or motion. Longet found only the posterior part of the medulla oblongata, the restiform bodies, the superior peduncles, and the upper part of the crura

cerebri to be sensitive; while the only portions, the disturbance of which is followed by an effect of motor power, are the anterior part of the medulla oblongata, the tuber annulare, and the lower part of the crura cerebri. In confirmation of the fact thus announced by Longet, we have certain experiments with electricity, by Matteucci, which show that so long as the current is confined to the upper part of the brain no effect on sensation or motion is produced, but as soon as the poles of the battery are carried to its base, sending a current through the deeper parts referred to by Longet, the animal cries out with pain, or its muscles are convulsed. While then we look alone to sensation, motion, and intellection as our sources of diagnosis, these observations of Longet and Matteucci indicate how disease of the brain may appear insidious by attacking portions of the organ not concerned in these functions. But as investigation traces more definitely the relations of the nerve centres with all functional activities of the organism, new external signs are afforded in the latter, of the process going on within the former; and it becomes less possible for serious organic lesion to invade the brain without showing itself to the practised eye in these outward manifestations.

Thus science, in proportion as it rises on this subject, illumines our path in every practical proceeding. As a further example, it throws a light upon facts in regard to the reflex function of the nervous system to explain in the most simple and satisfactory manner, phenomena that otherwise must be considered as utterly mysterious. The injurious effects resulting in some cases from the use of chloroform may be explained in this way. After examining the history of the accidental deaths from this agent, I suppose the errors committed in its employment may all be classed under three heads: 1st. Excessive administration; 2d. Too great exclusion of atmospheric air; and 3d. A too rapid impression on the system. A too sudden impression, by a quantity of chloroform otherwise safe,

has undoubtedly caused death in a number of instances, and neurological science shows, as I think, that the fatal effects have taken place in these cases by a reflex action of the nervous system, just as in cases of sudden death from a draught of cold water. The only explanation of the cause of death from cold drinks, as it occurs when the body is exhausted or overheated, is that the sudden excitation of the nerves of the stomach is transmitted through the "great sympathetic" to the spinal cord and thence through the par vagum to the heart, thereby paralyzing that organ and bringing the circulation to a stand. Brown-Séquard's experiments on dogs has demonstrated this; and he has also shown that the effects of chloroform in stopping the heart's action may be prevented by interrupting one of these channels of reflected excitation—that is, by section of the par vagum. The deaths reported as following the inhalation of a small quantity of chloroform are thus explained; whilst others, no doubt, were owing to the operator's ignorantly or recklessly excluding air from the lungs or prolonging the administration after the requisite anæsthesia was produced. Respecting excessive administration I would say, that I have found by the effect of chloroform on my own person that the nerve fibres which transmit painful impressions may be suspended in their function, while those answering to impressions of touch are but little affected; for the two sets are totally distinct; so that enough chloroform may be given to prevent the shock of pain before the subject is reduced to unconsciousness as to touch. And therefore the method of surgeons in carrying anæsthesia to complete unconsciousness, in cases of operations, is not required except when it is necessary to obviate the *dread* as well as the *feeling* of pain. In regard to the second cause of death referred to—the exclusion of air from the lungs —no remark is needed. As to the third, although we cannot resort to section of the nerve in the human subject, to prevent the too rapid impression of chloroform, yet if, considering the

nature of its action, we begin the administration so slowly and gradually that the nervous system, however susceptible, is calmed rather than violently impressed, no dangerous excitation will be produced—just as a man when exhausted by heat, if he will take iced water sip by sip, he may safely drink. This cautious plan of administering anæsthetics becomes necessary, always; because we are unable to say beforehand how liable the system may be to its dangerous reflex influence.

The fact is now well established that the tendency to violent reflex action varies much in different constitutions, and in the same constitution under different circumstances. Experiment has shown that when the spinal cord is congested by the action of strychnine, the slightest peripheral irritation will induce reflex convulsions; and, strange to say, the opposite state of depletion will develop the same tendency. This we see in the convulsions resulting from hemorrhage and the spasmodic movements so readily induced in decapitated animals. The law seems to be that where the controlling influence of the brain is in abeyance, or cut off, the reflex excitability of the spinal cord is developed whether the vessels of the latter are engorged or depleted.

If these opposite states of the bloodvessels obtain in different cases of reflex convulsions, we observe how important it is to distinguish them when selecting our remedies. In regard to narcotic remedies, we have some which increase the circulation of the nerve centres, such as opium, and others which reduce it, such as belladonna. In treating of the reflex influence of burns, Brown-Séquard seems to ignore the opposite states of the bloodvessels in different cases, and insists that opium is to be avoided as hurtful, while belladonna is to be employed But I have seen thirty drops of laudanum in an enema given to a little child, arrest convulsions of eight hours' duration, resulting from a burn, and the relief was so great and immediate that the value of the remedy could not be questioned.

So that I believe, with Handfield Jones, that there are cases of reflected irritation from burns "for which opium is the best remedy;" just as there is, no doubt, an opposite set for which other sedatives are best, depending on the vascular condition of the nervous centres.

If, again, the distinction here made be correct, it is important in its application to other cases, such as puerperal eclampsia. We all know what excessive bleedings have been advised and practised for these formidable convulsions. We know also that many patients have had eclampsia, as Tanner observes, associated with dangerous hemorrhage and the spasms increasing with the increase of "the flow;" while others, although bled *ad deliquium animi*, have continued in convulsions till relieved by delivery or an anæsthetic, or, perchance, by a dose of opium. The seizure in eclampsia is so like the lightning flash, as the word indicates, and so suddenly precipitates the patient's peril, that the physician, like the master of a vessel in a sudden storm, is required to act promptly; and the lancet has been regarded as the promptest remedy. But when it is remembered that the excessive reflex excitability which develops these convulsive movements in response to peripheral irritation in the uterus or elsewhere, is not so often dependent on congestion as on uræmic poisoning, then the lancet sinks in importance to a place secondary to chloroform and other anæsthetics, which both reduce excitability and allay the irritation of the peripheral nerves.

This reasoning may be considered too theoretical to be admitted as showing any benefit in practice to be derived from recent advances in neurology. But we are not restricted to theory. By the history of various nervous functional diseases, the deplorable disadvantages of former ignorance as compared with modern science, can easily be made apparent. Insanity is one of these diseases, and the statistics of our asylums show what is done, in one day, for its unhappy subjects. But

whilst I would award to these institutions the meed of praise I cannot indorse all that looks like science in the reports. These convey the idea that etiology in an asylum for the insane is an absolute science; that the cause is ascertained in every case with infallible certainty. Now I do not claim that our information in regard to the nerves goes exactly that far; but only that the modern treatment of insanity shows in its results, that with the culture of this branch of knowledge there has been a corresponding gain in its good fruit.

A similar illustration is afforded by the history of epilepsy —the "morbus sacer" of the ancients—the disease once considered too sacred for medical treatment, and therefore left to the care of the gods, though "the gods were often wicked and unfriendly." Until very recently, the treatment of this disease had no scientific basis. The nitrate of silver, so much employed, was an empirical remedy, the long use of which, by blackening the skin, only added a hideous deformity to a hopeless and distressing infirmity. But the experiments of Kussmaul and Tenner, showing the anæmic states of the brain in its connection with the insensibility and convulsion induced in rabbits; the observations of Brown-Séquard in regard to the epilepsy induced by experiments on guinea pigs, and the researches of M. Hall, Schrœder Van der Kolk, and Brown-Séquard into the pathology of this disease, have thrown such light upon many of its obscure points and revealed so much of its true character, as to place the treatment, before so exclusively empirical, now on quite a rational foundation.

I cannot omit the notice in this place of an illustration of the advantage of science in improving the treatment of nervous affections, supplied in the reports of one of the U. S. army hospitals established during the late civil war. It is well known that early in the war almost every hospital furnished patients suffering with nerve lesions, for whom medical officers seemed to have no means of relief, and often scarcely a cheer-

ing word of hope; and this because the works placed in their hands, and the text-books they had been accustomed to study, fell far short of the attainments of science on this subject. The Medical Department at Washington—then under the charge of Surgeon-General Hammond, with a most commendable regard for the welfare of the sick and wounded—provided wards in a General Hospital in Philadelphia for the special care and treatment of these invalid soldiers. The three gentlemen, Drs. Mitchell, Morehouse, and Keen, placed in charge, were selected from civil life, and well fitted for the position, both by their professional ability, and their devotion to the cause of the worthy, but unfortunate men, committed to their care. The success of this arrangement was such as to reflect the highest credit on the medical attendants, who were able to demonstrate the curability of cases which in other hospitals were, as Shakspeare says, "the mere despair of surgery," thereby securing the restoration of many who otherwise would have been doomed to the most hopeless and painful decrepitude. It is not necessary to detail the practice of these surgeons; but I may refer to their treatment of the nutritive changes associated with burning pain in these cases as entirely original; and to their report on the principles and method of employing electricity, proposed by Duchenne, as important to the profession in this country, because they are only incidentally alluded to in the works in our language. A knowledge of the therapeutical action of electricity has been a great desideratum in practice. It is a powerful agency—powerful for harm, if misapplied—and has too often been resorted to in nervous affections, with little apprehension of its remedial value.

But all such errors in practice will be rectified, as our knowledge on this subject increases and clears away the obscurities which still cloud many points in the complex structures and functions we have been considering. What an in-

centive to investigation would be the certainty of this increase of knowledge. Yet the prospect is enough, with many, to stimulate exertion, and prompt to every method of research.

But how may we expect new contributions to be made to our knowledge? In the last two volumes of *Transactions of the American Medical Association*, a writer—Dr. Haskell, of Massachusetts—has ingeniously sought, by a new generalization of facts previously observed, to unravel the mystery which inwraps the "double distribution of the cerebro-spinal nerves to muscles," and the functions of the "ganglia on their posterior roots." Although not admitting that the writer has satisfactorily sustained his theory, much less that he has disproved Sir Charles Bell's views concerning the roots of nerves, I refer to his method of reconsidering the facts which have come under the notice of different observers, and making new deductions, as well calculated to expose defects of former theories and to present the truth in a clearer light, or develope truth that, in a sense, is new.

By this method Dr. Campbell, another American writer, was led to conclude, from previously established facts, that there exists a distinct system of nerves which preside over secretion. The reflex phenomena of nutrition and secretion were known to physiologists before his paper appeared; but he is clearly entitled to the credit of first giving the explanation of them by the deduction of a distinct set of secretory and excito-secretory nerves. His doctrine, which was at once sustained by Marshall Hall, is one of extended application, and wherever it enables us to trace perversions of nutrition and secretion in disease to certain states of the nervous system, it affords us the most direct indications in practice. If, for example, such nutritive changes as ulceration of Peyer's glands in typhoid fever, or inflammation of the throat in scarlatina, can be ascribed to paresis of certain vaso-motor nerves (as they have been considered), we at once understand the necessity for the

supporting treatment which experience has pronounced the best, whilst many of the so-called alterative remedies are discarded as injurious medication.

But, whilst many important and intricate questions about the nervous system may be solved by facts already accumulated, we have unquestionably most to expect from new facts to be elicited by the rigid interrogation of nature and the careful observance of phenomena presented in health and disease. By the latter method, nearly all the recent advances referred to in this address have been made. Among the late conclusions reached in this way by Brown-Séquard is his startling revelation in regard to the numerous functional differences between fibres of the same nerve. He presents facts to prove that "besides the four distinct kinds of nerve fibres of the higher senses, there are eleven as distinct kinds in the spinal cord and in the cranial and other nerves." His discovery of the grouping in distinct columns in the spinal cord of four kinds of nerve fibres answering to the four varieties of sensitive impressions—touch, tickling, pain, and temperature —so that an injury to a small part of the cord may paralyze the power of recognizing any one form of sensation separately from the others, is founded on a demonstration which could not have been made from any previously recorded facts. The truth might have been suspected or the hypothesis formed from what was before known, but the conclusive proof was impossible. This, however, is not intended as an argument for hypothesis in medicine. If hypothetical reasoning were reliable, we might prove peculiar functional differences in the fibres of the optic, olfactory, and auditory nerves. If in accounting for achromatopsia or color-blindness we should suppose, as would be plausible, that each of the primary colors—red, blue, and yellow—has its own distinct nerve fibre to receive the impression it makes on the retina, then in paralysis or congenital defect of the fibre, which receives impressions from red colors, we would have

that form of color-blindness called ancrytheopsia; and another form which has been observed—acyanoblepsia—we would have those fibres affected that convey impressions made by the color of blue. The vague information we get from the books in regard to this strange affection is, that the cause is cerebral! How cerebral? The hypothesis here suggested would explain this if it could be considered reliable, which it cannot be without the demonstration of the distinct nerve fibres answering to each primary color. So might we reason in regard to other nerves of special sense. Finding an animal with a capability to recognize a scent not enjoyed by another, we might suppose a special nerve fibre adjusted to it; and so one of two ears hearing equally well, might, by a similar endowment, be an "ear for music." But to assume such things as true, without demonstration of the special fibres concerned in each faculty, is no better than the assumption of the Platonian philosophers who tell us of the "music of the spheres," but mortals have no ear adapted to its melody; nor the assumption we might make that there are odors in nature sweeter than the perfumes of "Carmel's flowery top," but man has no sense to smell them.

In this address, which must now be brought to a close, I have not presumed to instruct this learned Society by any attempt to offer new things. My object has been solely to awaken in the minds of the profession a deeper sense of the importance of the nervous system. Other branches of our science, I know, are of equal importance and are equally if not more progressive—for medicine is not a fixed creed—"is not fettered by a confession of faith," nor chained to any dogma. It is no miserable question of big or little doses, or this or that pathy, as the unthinking public is led to believe. It is an advancing science, aiming to explore the secrets not only of man's physical constitution, but of the properties of all agents by which it is influenced. And it is our high vocation, gentlemen, to study it more and more.

If we would be physicians, indeed—knowing nature as the term implies, and practising medicine according to the will of God, we must know the laws which his finger has inscribed for our guidance, not only on every structure of the human organism, but on all created forms, animate and inanimate, by which we are surrounded. This, it is true, involves the necessity for patient and laborious study in the light of medical philosophy. To those having the proper devotion, and following this light, success ultimately is secure; but ultimate and utter disappointment in reputation to those who pursue the *isms* and the *pathies*, which, like the Will o' the Wisp, shall only continue to mislead the ignorant and credulous until all such false and deceptive lights shall fade away beneath the blazing sun of Scientific Medicine.